newest-published text-books of the science is being bought by the public at the rate of a thousand copies a month. Under these circumstances it would be remarkable if all the works put before the public were of equal scientific merit, for such a demand cannot but tempt into the field the semi-scientific bookmaker who is ever ready to produce something to meet a popular taste. The work before us must, we fear, be classed with the semi-scientific. Its authors, so far as we are aware, are gentlemen who have yet to make their mark in the scientific world, and who, though not ill-informed in a general kind of way as to the applications of the science, cannot be said to have added by their present work to the scientific knowledge of the subject. The work opens with an account of the history of lighting in general from the days of Greece and Rome; and it devotes no inconsiderable part of its pages to the early history of electric lighting. We observe, by the way, that the authors fall into the error of putting Davy's discovery of the voltaic arc so late as the year 1813, when he experimented with his large battery of 200 cells. But he had discovered the arc at least nine years before that date. The manufacture of carbons for electric light claims half a dozen pages. Not too much when there is so much dependent on the quality of the carbon, and when carbons are as bad as they are. But we were not aware that those of M. Napoli were so superior to all others as to deserve a monopoly of description. The process of covering the exterior of the carbon-rods with an electrodeposited coating of copper is stated by the authors to have been first adopted in 1875 by M. Reynier, whose semi-incandescent lamp and modified Daniell's battery are described in effusive detail, though neither of these inventions can be said to be of capital importance. The chief feature in the book is that part which deals with the various systems of electric incandescent lamps. These are described very fully and with copious illustrations. The authors appear to prefer the system of Edison, for whom they have a great admiration, of whom they give a portrait (an honour shared by M. Gramme only), and concerning whom they narrate very naïvely several gossipy tales—how he and his assistants were nearly poisoned by mercury vapour when they first tried to work Sprengel pumps, and how he sent an expedition south for the metal thorium. The section devoted to dynamo-electric machines is also well illustrated, and fairly descriptive, though the style of exposition is of the "popular" order. The work concludes with a notice of the application of electric light to lighthouses, to naval and military warfare, and to the stage. With respect to the first of these applications, the authors attribute to Fresnel the application of dioptric lenses to lighthouses. Is it ignorance, or is it patriotic bigotry that is to blame for their obliviousness of the fact that Brewster suggested this very application in 1812, ten years before Fresnel, and that in 1820 he had already taken steps to urge the matter upon the notice of a too deliberate officialism? Many excellent woodcuts adorn the pages of the work of MM. Alglave and Boulard, which will doubtless make it a welcome book for many a library table where popular science is in request.

An Elementary Treatise on the Tides based upon that of the Late Sir J. W. Lubbock, Bart., F.R.S.; to which is added a newly-devised Method of Computation of the Heights of High Water at Liverpool, with Factors for other Ports, and Tables adopted by the Admiralty. By James Pearson, M.A., F.R.A.S. (London: J. D. Potter; Fleetwood: W. Porter and Son, 1881.)

THIS Treatise on the Tides, by the Rev. J. Pearson, M.A., F.R.A.S., contains an interesting historical sketch of tidal theories, extending from an early period to the present time; and while referring to the slow progress made in our knowledge of tidal phenomena, assures the inquirer of the interest attending the investigation. The researches of Newton, Bernouilli, La Place, and others, had gradually established a theory which, from the discussion of many observations made at ports in the United Kingdom by Sir J. Lubbock, brought into practical use a series of tables by which the times and the heights of high water at certain places, mainly on the shores of the United Kingdom, could be computed with an accuracy sufficient for the requirements of seamen, and others interested, especially the proprietors of docks. Based on the general results of Sir J. Lubbock's labours, the author, from observations extending over several years, has introduced tables auxiliary to those heretofore employed, for computing the heights of high water at Liverpool, where the tides have occasionally the great range of thirty-three feet. The results of these predictions (as compared with observation) show that the course of the "diurnal inequality"—previously disregarded—has by their aid been successfully traced. On the coasts of Great Britain generally, the diurnal inequality is not so important a factor as it is at Liverpool, at which place it amounts at times to one foot or more. The treatise cannot fail to be received with interest and to encourage attention to the subject.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.*]

The Movements of Jupiter's Atmosphere

The reference to the belts of Jupiter contained in my article on the geological activity of the tides (NATURE, vol. xxv. p. 213), was perhaps superfluous, for the subject is only collaterally connected with the points there under discussion; but as Mr. Mattieu Williams has commented on what I said, I should like to make a few remarks on his letter. Notwithstanding what he says I am still inclined to hold that the time-honoured explanation of the belts of Jupiter is the true one. In that explanation the terms trade and anti-trade winds are, I conceive, used in a somewhat extended sense as a consequence of thermal causes, and without reference to the existence of a solid nucleus, a current is supposed to set upwards in equatorial regions and then to spread out into higher latitudes; here the fluid has more moment of momentum than is adapted for the latitude in which it finds itself, and accordingly moves relatively to the subjacent matter in the direction of the planet's rotation, and forms an anti-trade wind. Conversely the trade winds arise from fluid moving into lower latitudes, when it has a deficiency of moment of momentum. Such an explanation seems to serve equally to explain the unequal rotation of the surface of the sun in different latitudes, and the Jovian belts.

The trade and anti-trade winds are essentially a thermodynamic effect, and in my paper I expressed an opinion that they might be partly due to the heat of the Jovian nucleus. It seems to be generally assumed that the great rapidity of that planet's rotation is a sufficient cause for the great violence of the supposed trade-winds which produce the belts. But my chief object in referring to the matter was because rapidity of rotation is not a sufficient explanation, without a statement as to the mode of reinforcement of the thermodynamic causes. Now the great distance of Jupiter from the sun largely weakens those causes, and it seems to me that there are only two ways in which they can be strengthened, viz. first by the large amount of gas on which the solar radiation has to work, and secondly, by the heat of the nucleus.

With regard to the deductions to be drawn from the low specific gravity of Jupiter, I may mention that in 1876 I pointed out that the observed ellipticity of the planet's figure can only be explained on the assumption of great density of the central portions of the planet. Taking indeed the best data attainable, I showed that the mean density of Jupiter must be about 70 times as great as the superficial density, if we follow Laplace

as to the nature of the law by which the density increases internally.[1] In the article in *Nature*, I adduced the argument on which Mr. Williams comments, as a slight corroboration of the conclusions as to the physical constitution of the planet, which have been derived from telescopic inspection, and from observation of the ellipticity of figure.

From the latter part of Mr. Williams's letter I must beg leave to dissent. If one were to describe the oceanic tides on the earth as a reeling motion of the solid earth within the sea, it would surely be a somewhat obscure description of the facts, but the reeling of the Jovian nucleus *can* only be a tidal phenomenon.[2] Now the masses of the Jovian satellites are so small, that they can only raise very small tides, except indeed on one hypothesis, of the truth of which we have no evidence, and which would not tend to explain the belts if it existed. The tide raised by a small satellite can only be large when the "free" period of oscillation of the gaseous or liquid ocean is nearly the same as the "forced" period. If this were the case with one of Jupiter's satellites, it certainly would not be so with the others. Although tides accompanied by *fluid friction* do tend to produce a longitudinal current adverse to the planetary rotation, yet no current of a millionth part of the velocity requisite for the production of the belts could possibly be occasioned by the tidal friction due to Jupiter's satellites.

For these reasons I quite dissent from Mr. Williams's explanation of the belts, and of the unequal solar rotation.

Sir William Thomson has recently pointed out, in a paper read before the Physical Society of Paris, a probable cause of the reinforcement of an atmospheric tide in the earth, due to an approximate agreement of free and forced periods of oscillation. He remarks that the semi-diurnal constituent of the barometric oscillation is nearly everywhere very much larger than was to be expected, and he shows that the sun and earth together constitute a thermodynamic engine whereby the earth's rotation is accelerated. Rough numerical calculations are given, wherefrom it appears that the amount of this acceleration may not be entirely negligeable, when we consider the degree of refinement to which modern astronomy has arrived. G. H. DARWIN

R.M.S.S. *Medway*, Southampton, Feb. 2

The Search for Coal under London

IN a recent communication to this journal I dwelt upon the importance of a systematic search being made for the Carboniferous rocks under London, by a series of borings running from north to south, and only a few miles apart; but I pointed out at the same time that much of the expenditure required for such a search might be saved by a judicious selection of sites for the first two or three borings. I then quoted the opinions of Mr. Godwin-Austen and Prof. Prestwich as to the localities at which such explorations might be undertaken with the greatest chance of success. My friend, Prof. Prestwich, has written to me expressing general agreement with the views I have put forward on the subject, but calling my attention to some other suggestions of his as to the points at which borings might be executed, with fair hopes of success. Writing in the Reports of the Coal Commission in 1870 (p. 162), Prof. Prestwich expressed himself as follows:—

"The direction of the great underground coal trough is, we think, likely to be on a line passing through North Wilts, Oxfordshire, thence across Hertfordshire, South Essex, the north-east extremity of Kent, onwards towards Calais, near to which place it is thrown out by the rise of the underlying rocks, but resumes again at Thérouanne. Or in case of the anticlinal axis taking a more southern course we should look for the coal basin or basins along a line passing from Radstock, through the Vale of Pewsey, and thence along the North Downs to Folkestone and near to Calais."

Some years later Prof. Prestwich wrote as follows:—

"In short, while there is every reason to hope that on the *south* of London we may yet find in the *Lower Greensand*, beneath the Tertiary Strata and Chalk, a source of large and valuable water-supply for metropolitan purposes, there is strong reason to believe in the probability of the discovery to the *north* of London of *Carboniferous Strata*, including possibly productive Coal-measures." . . . ("On the Range of the Lower Greensand and Palæozoic Rocks under London," by J. Prestwich. From *Quart. Journ. Geol. Soc.* for November, 1878, p. 911.)

The discovery of Upper Devonian strata, both at Turnford and at Tottenham Court Road, in both cases dipping at high angles, lends not a little support to the view that a trough of Carboniferous strata may exist between those two localities. Prof. Prestwich authorises me to state that what he would now recommend would be a boring "a mile or two north of Kentish Town, not directly north, but north-east or north-west, so as to avoid the hills—say about Edmonton on the one side, or near Edgware on the other." On the south side of London he would prefer to avoid the Lower Greensand, and would recommend a boring "just beyond its outcrop at Red Hill—somewhere between there and Horley." But he thinks that if Coal-measures were found to extend beneath the Lower Greensand, means might be found to sink through the latter, by the new appliances of which the Belgian engineers have so largely availed themselves. JOHN W. JUDD

Researches on Animals containing Chlorophyll

1. DR. BRANDT'S observations (*Sitz. d. Berlin Physiol. Gesellsch.*, Nov. 11, 1881) are upon the green bodies of *Hydra*, *Spongilla*, a fresh-water planarian, and numerous infusors. He finds that these green bodies are masses of hyaline protoplasm, containing a nucleus and a chlorophyll-granule. Sometimes two to six are present, these he considers are states of division. He regards these facts as proving that those bodies are unicellular algæ, and erects the genus *Zoochlorella*. He finds them survive isolation, and even develop starch in light. Specimens from Spongilla were taken in by infusors, but were either digested or ejected: those from a dead Hydra were, however, retained by *Paramœcium*, *Coleps*, &c. He believes that the chlorophyll never belongs to the animals, but always to algæ.

My observations deal with the yellow cells of quite different animals. I have, however, ventured the opinion that in most of the above cases, the green bodies do belong to the animals, and are not algæ, and I do not yet see sufficient reason for withdrawing that view.

2. For the yellow cells of Radiolarians and Cœlenterates (for the alga nature of which Dr. Brandt so ably argued in his former paper) he proposes the genus *Zooxanthella*. Here Dr. Brandt has doubtless priority.

3. He observes that large Radiolarian colonies show no signs of digesting foreign bodies, that these and also Spongilla can be kept best in filtered water, and that the latter will not live in a half darkened room. These facts are doubtless new.

4. Dr. Brandt concludes that the algæ maintain their hosts; that so long as the animals contain few or none, they feed in the ordinary way, but when sufficient algæ are present, they are nourished like plants. He indicates an analogy to lichens (an hypothesis which, as I also state in my paper, was first ventured by Semper), and yet points out a distinction, since in a lichen there is an association of an alga with a true parasite, here a "Symbiose" of algæ with animals accustomed to independent life, which they, however, give up, and take in no further nutriment. Thus in a morphological sense the algæ, in a physiological sense the animals are the parasites.

While welcoming Dr. Brandt's interesting paper, and while not desiring to lay too much stress on such awkward facts for his view as that *Hydra*, *Anthea*, *Velella*, &c., are quite as voracious as their congeners unprovided with chlorophyll, or that the animal may possess its chlorophyll from development, and while giving him and his predecessors all due credit for their valuable observations and theoretic insight, I must point out that (1) the *demonstration* of the truth of the view that the yellow cells of Radiolarians and Cœlenterates are algæ, (2) the development of the hypothesis of the lichenoid nature of the alliance between alga and animal into a theory of mutual interdependence, and (3) the transference of that view from the region of probable speculation into that of experimental science, remain with my paper. For it will not do to ignore, with Dr. Brandt, such weighty opposing evidence as (1) the recent direct statement of Hamann that the yellow cells of Cœlenterates are not algæ, but unicellular glands, (2) the observation of Krukenberg that *Anthea viridis* did not evolve oxygen, or (3) the failure of himself and others to prove the presence of cellulose and chlorophyll, or even to

[1] Monthly Notices of R.A.S. Dec. 1876, "On an Oversight in the Mécanique Céleste, and on the Internal Densities of the Planets."

[2] The expression "reeling" would at the first glance lead one to suppose that a diurnal tide is referred to, in which the fluid parts are carried relatively to the nucleus in the direction of the disturbing satellite, but without change of superficia form, technically a spherical harmonic deformation of the first order. But it is well known that this class of displacement must be non-existent, and therefore it must be presumed that Mr. Williams does not intend this.